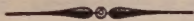


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ESP EXPERIMENTS WITH CLOCK CARDS A NEW TECHNIQUE WITH DIFFERENTIAL SCORING

G. W. FISK AND A. M. J. MITCHELL

Ordinary card guessing a Call is either a complete success or a total failure. It was thought it might be interesting to contrive a method whereby it would be possible to appreciate degrees of success. A simple analogy is that of rifle-shooting. In firing at the target one may score a 'bull's-eye', a complete success. But 'inners', 'outers', and 'magpies' also count in a varying degree in the total score. Obviously a marksman who was only allowed to count the 'bulls' would be at a disadvantage compared with one who could also score with 'near-misses' and 'not-so-near-misses'. Can it be shown by suitably devised ESP experiments whether extra-chance scores are obtained by a combination of hits on the target and others at varying distances from it? Are they obtained by the consistent scoring of 'near-misses'? Do some subjects achieve significant scores by the first method and others by the second? Do some subjects tend to score by means of 'bull's-eyes', and when not doing so to miss the target by a wide margin? What sort of 'grouping' is made by those who consistently score below chance expectation? These are some of the questions that suggest themselves, but which cannot be studied by the hit-or-miss technique hitherto used in ESP experiments. If they can be answered, perhaps some light may be shed on the spontaneous operation of ESP.

Basis of Method

If there were a row of dots one of which, the target, was encircled—

. . . ○ . . .

the proximity to the target-dot of the dot guessed by a percipient would be a measure of a varying degree of success or failure.

J.B. did not often achieve other than chance results. Previously both girls had been tested with ESP cards but without any success whatever.

The trials began in April 1951 and continued on Sunday afternoons with occasional breaks until August 1952. Latterly the game lost its freshness and the participants became rather bored. There was a distinct decline in S.B.'s scores. The results are summarized both for 'Direct Hits only' (the hit-or-miss method of ordinary ESP cards) and also for Differential or Divergence Scoring.

We have used the symbol t for the Critical Ratio throughout this paper in order to conform with normal statistical practice. The t values given by the differential method of scoring are larger than those given by the ordinary direct-hit scoring which suggest a greater sensitivity in the former method (see Appendix 2).

An analysis was made to determine which of the seven possible divergences were responsible for the deviations from chance expectation.

TABLE 2
DISPERSION OF DIVERGENCE HITS

PERCIPIENT S.B.

	Δ_0	Δ_1	Δ_2	Δ_3	Δ_4	Δ_5	Δ_6
Total Hits	161	269	224	246	221	222	99
MCE -	120	240	240	240	240	240	120
Dev. -	+41	+29	-16	+6	-19	-18	-21
t - -	3.91	2.05	1.13	0.43	1.34	1.27	2.005
P - -	9.2×10^{-5}	0.04	0.26	—	0.18	0.20	0.045

PERCIPIENT J.B.

	Δ_0	Δ_1	Δ_2	Δ_3	Δ_4	Δ_5	Δ_6
Total Hits	125	280	259	247	238	252	123
MCE -	127	254	254	254	254	254	127
Dev. -	-2	+26	+5	-7	-16	-2	-4
t - -		1.79			1.10		
P - -		0.073			0.27		

It will be seen that S.B.'s overall significant score is due mainly to her large positive score on Δ_0 and a smaller positive score on Δ_1 . The scores on Δ_2 , Δ_3 , Δ_4 , Δ_5 , and Δ_6 are all smaller and, with one exception, negative. An excess of direct hits and of near-misses superimposed upon an otherwise chance series would naturally cause a relative deficit of larger divergences, so S.B.'s results are in keeping with the hypothesis that direct hits and near-misses comprised her primary psi effect. J.B. owed her small positive score chiefly to an excess of near-misses, as shown by her positive score on Δ_1 , but the effect is insufficiently marked to produce statistical significance.

It was realized that, whilst these first GESP experiments with the new technique provided some interesting information, the conditions under which they were conducted were by no means as rigid as is necessary for firm conclusions to be drawn. In all GESP experiments, unless controlled with the utmost care, there is always the fear that *indicia* of one sort or another may have been given unconsciously to the subjects. So, when a supply of Clock cards became available, a series of more refined experiments was started with percipients living in different parts of the country. Most of these had already taken part in the Society's 'Home-Testing' ESP experiments using ordinary ESP cards and methods. Not one had shown any consistent scoring above chance expectation.

3. DTSP trials

The method now adopted was DTSP, i.e. Down Through Sealed Packs. Packs of 12 Clock cards were arranged by G.W.F. in random order by means of random numbers. These were sealed in envelopes in such a way that they could not be opened inadvertently without leaving traces of the interference. The top of the pack was indicated on the envelope. The sealed packs were distributed in sets of four runs at a time (48 trials) from May 1951 to October 1952 to 54 percipients. They were to call the hour depicted on each card from top to bottom of the pack. Score sheets and *unopened* packs were then to be returned to G.W.F. for assessment. The grouped results showed a highly significant positive deviation from chance expectation.

After a detailed examination of these results it was decided that it was probably inadmissible so to group the results for all percipients. (To complete the record a summary of such a grouping is given in Appendix 3). In the majority of cases a group of percipients—two, three, or more—made their calls on the *same* sealed pack because the subject to whom the packs were sent in the first place enlisted friends or relations who separately recorded their guesses. Now this may involve what is known as the 'stacking effect', in this case the result of two factors. First, it has been found that callers show distinct preferences in the hours guessed (see Appendix 4). Second, the random order of the cards entails that, for a limited number of trials, the different card values are not equally distributed. The coincidence of these two factors may produce a 'stacking effect' which, to a greater or less degree, may invalidate the probability figures arrived at by the usual methods of assessment. It may increase or decrease these figures and although the effect can hardly be very large it must be taken into account. Unfortunately the mathematical problems

involved for dealing adequately with it for Clock Divergences have not yet been solved.

Pending a solution to these problems it was decided to consider only those primary subjects (17 in number) to whom separate packs were sent, and to ignore the scores of any enlisted secondary subjects. One of this smaller group had a particularly outstanding result, so the following summary is divided into two parts :

TABLE 3
SUMMARY OF NON-STACKING DTSP RESULTS

16 PERCIPIENTS

Runs = 236

Trials = 2832

		Direct Hits	Sum of Divergences
		Δ_0	Δ_0 to Δ_6
Total	- -	247	8182
MCE	- -	236	8496
Dev.	- -	+11	+314
<i>t</i>	- -	0.747	3.32
<i>P</i>	- -	0.45	0.0009
Odds	- -	—	1,100 : 1
PERCIPIENT S.M.			
Runs = 20			
Trials = 240			
Total	- -	40	573
MCE	- -	20	720
Dev.	- -	+20	+147
<i>t</i>	- -	4.67	5.33
<i>P</i> (approx.)	- -	3.4×10^{-6}	1.0×10^{-7}
Odds (approx.)	- -	294,000 : 1	10 million to 1

The analysis to determine which of the seven possible divergences are responsible for the overall results follows in Table 4

TABLE 4
DISPERSION OF DIVERGENCE HITS

16 PERCIPIENTS (non-stacking)

		Δ_0	Δ_1	Δ_2	Δ_3	Δ_4	Δ_5	Δ_6
Hits	- -	247	540	466	479	438	451	211
MCE	- -	236	472	472	472	472	472	236
Dev.	- -	+11	+68	-6	+7	-34	-21	-25
<i>t</i>	- -	0.747	3.43	0.30	0.35	1.71	1.06	1.70
<i>P</i>	- -	0.45	0.0006	—	—	0.09	0.29	0.09

(Chi Square = 19.214. $P = 0.0062$ approx. Odds 160 : 1)

PERCIPIENT S.M.

		Δ_0	Δ_1	Δ_2	Δ_3	Δ_4	Δ_5	Δ_6
Hits -	-	40	49	47	34	33	26	11
MCE -	-	20	40	40	40	40	40	20
Dev. -	-	+20	+9	+7	-6	-7	-14	-9
t -	-	4.67	1.56	1.21	1.04	1.21	2.43	2.10
P -	-	3.4×10^{-6}	0.12	0.23	0.30	0.23	0.015	0.036

(Chi Square = 38.567. $P = 1.16 \times 10^{-7}$ approx. Odds 10 million to 1)

The 16 group made by far the biggest number of hits on Δ_1 with Δ_0 a poor runner-up, i.e. the 'near-misses' were worth far more than the 'bull's-eyes'. This is the same effect as was noted on a smaller scale in the data of the subject J.B. (see Table 2). S.M.'s deviations, on the other hand, were due almost exclusively to an excess of direct hits. It is, of course, the positive deviations on the left-hand side of the table coupled with the negative deviations on the right-hand side that yield the overall significant Divergence t . The Chi Square figures give a measure of the significance of the general dispersion of the seven classes.

4. Discussion and Conclusions

It is considered that these preliminary results are encouraging and justify further work on the Clock technique. Tentative conclusions are drawn below and suggestions made towards refining the appraisal method.

(a) The tests reported here have been appraised by two methods—first by taking into account direct hits only and secondly by using divergences. The latter differential method gives rise to higher t values than the direct hits method. Now, the differential t is not independent of the direct hits t (for a particular test) since the total divergence depends on the number of direct hits as well as near-misses, etc. Thus, further statistical work is required to determine the significance of the *difference* between the two t values.

(b) Subject to the outcome of the above work, it does appear that an Angular Error effect exists. This appears similar to that which would occur in the visual perception of the hour hand of a clock viewed under conditions of poor visibility. Possibly the functioning of the psi faculty is conditioned by some psychic equivalent of myopia or even of cataract! While the present system of scoring seems to take account of the effect, the observed dispersion of divergences suggests that a more efficient valuation might be possible. For example, the relative unimportance of Δ_2 and Δ_3 is noticeable with practically all subjects. Perhaps the most important advantage of differential scoring over the usual 'hit-or-miss' method will lie in this ability to distinguish between

the various ways in which percipients achieve a significant score (either positive or negative), e.g. the excess or deficit may be due to the number of direct hits only or entirely due to some particular value of a near miss or relative failure.

(c) A method is required for the appraisal of group tests where the same pack of cards is used by a number of percipients. A possible line of attack is the extension of Greville's method (*Ann. Math. Statistics*, XV, 1944, 432-4) to include differential scoring.

(d) Guessing the time seems to suit some percipients better than the use of geometrical symbols as with ordinary ESP cards. This may be related to the fact that we all read clocks and watches many times a day and it is about the first angular spacing that children learn to appreciate. Although most of the subjects had already been tested with ordinary ESP cards with null results, a relatively small number of clock card trials was sufficient to produce a clearly significant psi effect. Maybe the novelty of the idea helped, but it is interesting that of 54 people tested one, S.M., proved to be what might be called a 'star' subject. In 240 trials S.M. achieved a direct-hit score of 4.67 standard deviations. To produce an equivalent t value in 240 standard ESP card trials, an average of 8 hits per run of 25 would need to be maintained. In actual fact, in S.M.'s previous trials with standard ESP cards the results were null.

(e) It has been found that subjects do not record each of the target hours with equal frequency. Significant deviations are found for certain hours but the particular hours preferred—or avoided—vary from subject to subject. Taking the results as a whole, the hours II, III, VII, and IX are preferred while VIII, X, XI, and XII are avoided. The effect of this aberrant calling is to reduce the maximum potential score of a percipient and, if taken into account, is expected to reduce the quoted P values and hence increase the significance of the results. The required corrections to the P values have not been computed and would only be of importance when examining marginal differences between t values.

We may add that a form of differential scoring is also applicable to PK trials with dice, and some experiments have been made with not uninteresting results. It is hoped to make a report shortly.

Finally, we are grateful to all those who have assisted in these experiments, particularly to Dr D. J. West who gave constant advice and encouragement and who, indeed, first suggested to us the possibility of differential scoring; to J. Fraser Nicol for so enthusiastically tackling the primary mathematical problems involved; and to our patient subjects who submitted so readily to the tests imposed.

APPENDIX I

DISTRIBUTION OF TOTAL DIVERGENCE IN THE CLOCK CARD TEST

1. *Introduction*

In a Clock card test of n trials, the problem is to determine the significance of the deviation of an observed total divergence Δ from the mean chance expectation. The first step is to determine the nature of the distribution of total divergence, i.e. the relation between any particular divergence and its probability of occurrence. It is desirable to design experiments so that this distribution closely approximates to distributions for which significance tables are available. The Normal distribution is particularly convenient.

A derivation of the distribution for Clock cards is outlined below and compared with both the Normal and Binomial distributions. The latter is the basis of most ESP and PK tests.

2. *Probability distribution of total divergence*

The method of derivation is based on the use of generating functions (g.f.).¹ In this case,

$$\text{g.f.} = [p_0 u^0 + p_1 u^1 + p_2 u^2 + p_3 u^3 + p_4 u^4 + p_5 u^5 + p_6 u^6]^n$$

Here, $p_0, p_1 \dots p_6$ are the probabilities of divergences 0, 1...6, arising from a single call. In the expansion of this function, the probability of a particular total divergence Δ arising out of n trials, is the coefficient of u raised to the power Δ .

The desired distribution may be recognized from the g.f. as being a Multinomial which is known to approximate to the Normal.

Substituting for the known values of $p_0, p_1 \dots p_6$, in the above g.f., we have:

$$\text{g.f.} = [u^0/12 + 2u^1/12 + 2u^2/12 + 2u^3/12 + 2u^4/12 + 2u^5/12 + u^6/12]^n$$

Next we set up the factorial moment g.f. by writing $(1 + \alpha)$ for u in the g.f. After re-arrangement

$$\text{f.m.g.f.} = [1 + 3\alpha + 55\alpha^2/12 + 25\alpha^3/6 + 9\alpha^4/4 + 2\alpha^5/3 + \alpha^6/12]^n$$

The mean chance expectation of total divergence is the coefficient of α when the f.m.g.f. is expanded. We find,

$$\text{MCE} = 3n.$$

It is important to derive the standard deviation and also to determine the discrepancies between this distribution and the Normal. We therefore set up the f.m.g.f. about the mean as follows:

$$\text{f.m.g.f. about the mean} = (1 + \alpha)^{-3n} (\text{f.m.g.f.}).$$

Substituting for the f.m.g.f. this expression reduces to

$$[1 + (19\alpha^2 - 19\alpha^3 + 27\alpha^4 - \dots)/12]^n$$

¹ See, for example, A. C. Aitken, *Statistical Mathematics*, (Edinburgh, Oliver & Boyd).

and must be expanded in order to pick out the co-efficients of $\alpha^2/2!$, $\alpha^3/3!$, $\alpha^4/4!$ from which we find the moments μ_2, μ_3, μ_4 . These moments will be sufficient to estimate the normality of the distribution. On expansion we find :

$$1 + \frac{19n}{6} \cdot \frac{\alpha^2}{2!} - \frac{19n}{2} \cdot \frac{\alpha^3}{3!} + \frac{n}{12} \cdot (287 + 361n) \cdot \frac{\alpha^4}{4!} + \dots$$

The co-efficients of $\alpha^2/2!$, $\alpha^3/3!$, $\alpha^4/4!$ are designated $\mu_{(2)}, \mu_{(3)}, \mu_{(4)}$ where,

$$\mu_2 = \mu_{(2)} = 19n/6.$$

Thus

$$SD = \sqrt{(19n/6)}$$

Also

$$\mu_3 = \mu_{(3)} + 3\mu_{(2)} = -19n/2 + 19n/2 = 0.$$

Hence the Co-efficient of Skewness, $\beta_1 = \mu_3^2/\mu_2^3$, is zero and the distribution is symmetrical about the mean.

$$\begin{aligned} \mu_4 &= \mu_{(4)} + 6\mu_{(3)} + 7\mu_{(2)} = n(287 + 361n)/12 + 133n/6 \\ &= n(553 + 361n)/12. \end{aligned}$$

The Co-efficient of flattening, $\beta_2 = \mu_4/\mu_2^2$ is given by

$$\beta_2 = 3(553/361n + 1)$$

It will be noticed that β_2 exceeds 3 (the value for the Normal distribution) by an amount which diminishes as n increases. The implication is that the divergence distribution is taller and slimmer at the centre than the bell-shaped Normal i.e. it is leptokurtic. However, it tends to the Normal as n increases and is symmetrical for all values of n —as is the Normal.

3. The Binomial Distribution

This is a well-known distribution which is a special case of the Multinomial and also tends to the Normal as n increases. The g.f. is $(pu^1 + qu^0)^n$, where p and q are the probabilities of a hit and a miss respectively and n is the number of trials. As before, the probability of h hits is the co-efficient of u raised to the power h in the expansion of the above g.f. Since the distribution is well established, we quote the parameters as follows :

$$MCE = np$$

$$SD = \sqrt{(npq)}$$

$$\mu_3 = npq(q-p)$$

$$\mu_4 = npq[p^2 + (3n-4)pq + q^2]$$

$$\text{Coeff. of Skewness} = (q-p)^2/npq$$

$$\text{Coeff. of Flattening} = 3 + [(p^2 + q^2)/pq - 4]/n.$$

4. Comparison of the Normal, Binomial, and Clock distributions

The tests described in the main body of the paper have been appraised using direct hits (binomial distribution) and divergences (clock distribution). It is of interest to substitute the appropriate values of p and q , ($1/12$ and $11/12$ respectively), in the parameters of the binomial and compare with the Clock distribution :

Parameter	Normal	Binomial	Clock
MCE	—	$n/12$	3^n
SD	—	$\sqrt{(11n/144)}$	$\sqrt{(19n/6)}$
Skewness	0	$100/11n$	0
Flattening	3	$3 + 78/11n$	$3 + 1659/361n$

Thus the Clock distribution is more Normal than the corresponding Binomial, but the difference is negligible for most purposes when n is large—say, 100 or more.

APPENDIX 2

1. Introduction

For any Clock card test, two values of t can be derived—one from the total number of direct hits, the other by totalling divergences. In general, these two values will differ and it is the purpose of this Appendix to examine the possible extent of the difference.

We consider a general case where h direct hits have been scored in n trials. The total divergence on the same test is Δ .

2. Direct Hits t

$$\text{Now } t = \frac{\text{Deviation}}{\text{SD}}$$

Using the data from Appendix 1,

$$t = \frac{h - n/12}{\sqrt{(11n/144)}}$$

It is convenient to express the number of direct hits as a fraction, k , of the total possible i.e. n . Hence,

$$h = kn.$$

$$\text{Then } \frac{t}{\sqrt{n}} = \frac{k - 1/12}{\sqrt{(11/144)}} = \frac{12k - 1}{\sqrt{11}}$$

3. Divergence t

$$t = \frac{3n - \Delta}{\sqrt{(19n/6)}}$$

$$\text{Or } \frac{t}{\sqrt{n}} = \frac{3 - \Delta/n}{\sqrt{(19/6)}}$$

We now examine the possible values of total divergence. Noting that the divergence for a direct hit is zero, there are $(n - h)$ trials for which the individual divergence is not zero. Thus the minimum possible total divergence is $(n - h)$, all the trials which are not direct hits having divergence 1. Similarly, the maximum total divergence will be $6(n - h)$. Thus t can be written as :

$$\frac{t}{\sqrt{n}} = \frac{3 - [(n - h) \text{ up to } 6(n - h)]/n}{\sqrt{(19/6)}}$$

Introducing the parameter k , we have :

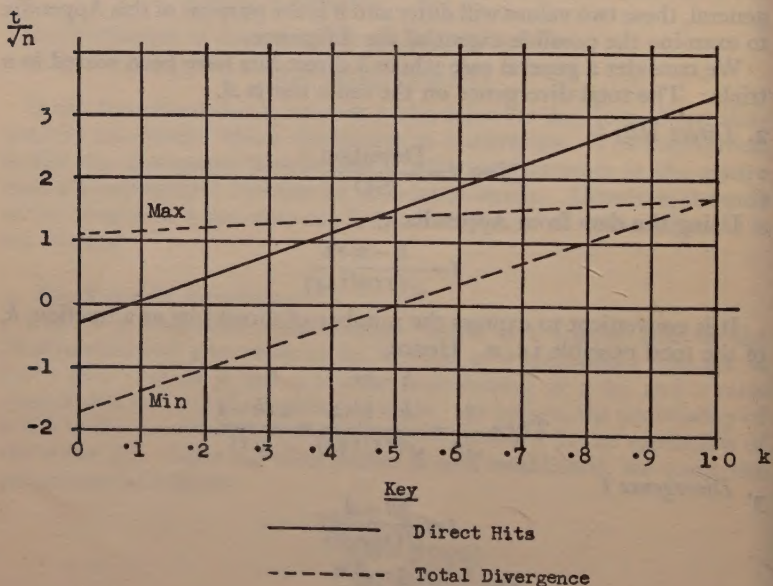
$$\frac{t}{\sqrt{n}} = \frac{3 - [(1-k) \text{ up to } 6(1-k)]}{\sqrt{(19/6)}}$$

Hence for a particular k the value of t/\sqrt{n} must lie within the range :

$$\frac{2+k}{\sqrt{(19/6)}} \text{ and } \frac{6k-3}{\sqrt{(19/6)}}$$

4. Comparison of t values

The t values for the direct hit and divergence scoring systems are compared in Fig. 2. It will be noted that when k is small, the usual state of affairs in ESP and PK tests, the divergence t can lie within a wide range of values astride the direct hits t value. Thus the divergence system of scoring is potentially more sensitive over the range of practical interest.



Further statistical work is indicated to determine the significance of the difference between the direct hits and divergence t values. The problem is complicated by the fact that the two t values are neither independent nor mutually exclusive.

APPENDIX 3

SUMMARY OF DTSP TRIALS. ALL PERCIPIENTS (54)

Runs = 516

Trials = 6192

		Direct Hits	Sum of Divergences
		Δ_0	Δ_0 to Δ_8
Total Hits	-	576	17902
MCE	-	516	18576
Dev.	-	+60	+674
t	-	2.75	4.81
P (approx.)	-	0.006	1.6×10^{-8}
Odds (approx.)		170 : 1	625,000 : 1

DISPERSION OF DIVERGENCE HITS

	Δ_0	Δ_1	Δ_2	Δ_3	Δ_4	Δ_5	Δ_6
Hits	576	1117	1041	1050	968	960	480
MCE	516	1032	1032	1032	1032	1032	516
Dev.	+60	+85	+9	+18	-64	-72	-36
t	2.74	2.90	0.31	0.61	2.18	2.46	1.64
P	0.006	0.0037	—	—	0.029	0.014	0.100

(Chi Square = 29.880. $P = 2.1 \times 10^{-5}$ approx. Odds 48,000 : 1)

APPENDIX 4

HOUR PREFERENCES IN PERCIPIENTS' CALLS

In examining the Clock test data, considerable variation was noticed in the number of times percipients recorded each of the target hours I to XII. The following table shows the number of times each hour was recorded by S.B., J.B., and the group of 54 percipients. The frequency of occurrence for each hour is given as the deviation from the mean chance expectation. As there were always 12 calls to a run, the MCE for each hour is equal to the number of runs.

Percipient	S.B.		J.B.		54 Group		All P's	
No. of runs	120		127		516		763	
Hour	Dev.	t	Dev.	t	Dev.	t	Dev.	t
I	+26	2.48	-19	1.76	+12	0.55	+19	0.72
II	+10	0.95	+39	3.61	+71	3.21	+120	4.53
III	+68	6.47	+3	0.28	+31	1.42	+102	3.85
IV	-5	0.48	+18	1.66	+40	1.84	+53	2.00
V	-53	5.05	+1	0.09	+13	0.60	-39	1.47
VI	-5	0.48	+8	0.74	+24	1.10	+27	1.02
VII	+3	0.29	+38	3.52	+62	2.85	+103	3.89
VIII	-52	4.95	-36	3.33	-15	0.69	-103	3.89
IX	+21	2.00	+51	4.71	+15	0.69	+87	3.28
X	+3	0.29	-54	5.00	-38	1.74	-89	3.36
XI	-19	1.81	-13	1.20	-111	5.10	-143	5.40
XII	+3	0.29	-36	3.33	-104	4.78	-137	5.17
χ^2	98.3		94.6		72.4		139.5	
t	9.48		9.54		7.47		12.1	

As values of P for such high χ^2 values were not available the corresponding t values have been computed to provide strictly comparable estimates of significance. There is, of course, no doubt that the frequency of occurrence of the individual hours called is far from normal. The hours for which considerable deviation occurs vary from percipient to percipient, but taking the results as a whole, the hours II, III, VII, and IX are preferred while VIII, X, XI, and XII are avoided. It was not thought worthwhile at this stage to investigate the reasons for such aberrant calling but some consideration has been given to the problem. Although the targets are based on a clock face, they also constitute a series of numbers so that an investigation should not be restricted to a percipient's reactions to specific times of day. It seems likely, for example, that the hours X, XI, and XII might be avoided because they require writing two figures instead of only one for the remaining hours. Percipients may have favourable—or unfavourable—reactions to certain numbers; some persons consider the numbers 3 and 7 to be lucky and might therefore record them more frequently. It is anticipated that the reasons for aberrant calling will be complex and would be best approached from the psychological standpoint.

One effect of aberrant calling will be to limit the maximum potential score of a percipient, which is undesirable. However, it is considered that this disadvantage is offset by the apparent ability of percipients to score more highly on Clock tests than on those employing ESP cards. A further effect is that the calculated standard deviations are expected to be somewhat greater than those derived taking into account the variation from hour to hour of the probability of a hit. Thus the quoted P values are probably too high. The corrections to be applied have not been calculated for the Clock tests, but they might be necessary when examining marginal differences between direct hits and divergence t values.

HOME-TESTING ESP EXPERIMENTS

AN EXAMINATION OF DISPLACEMENT EFFECTS

BY D. J. WEST

Two brief reports on the 'Home-Testing' experiments organized by G. W. Fisk have already been published in this *Journal* (January–February and November–December 1951), but for the benefit of readers who have not seen them here is a short recapitulation of their origin and conduct.

The project, undertaken by Mr Fisk at the request of the S.P.R. Council and begun in January 1950, was intended prim-

arily to discover, by means of a large-scale search, some consistently high-scoring ESP subject. Since any promising subjects would have been re-tested under carefully controlled conditions, the initial tests were informal. An instruction leaflet explaining how to conduct tests under GESP conditions, together with a supply of ESP cards and score sheets, was sent by Mr Fisk to as many people as could be interested in the matter. Thereafter they were left to carry out the tests in their own homes with the request that they send every completed score sheet to G. W. F. In these home tests the cards were hand-shuffled and there were no special checks upon possible recording errors. The instruction leaflet recommended that agent and subject should be separated by a screen at least three feet square or by its equivalent, and that the experimenter should signal by tapping when the agent looked at each card, but there is no means of knowing how closely these conditions were followed.

SUMMARY OF RESULTS

Experimenters were asked to carry out when possible eight or sixteen runs through the pack of twenty-five cards with each of their subjects in the first instance, and to give further tests with those subjects who produced promisingly deviant scores. A total of 236 subjects was tested, but only ninety-seven of them completed sixteen or more runs. None of the subjects produced consistently outstanding scores with the exception of Mr B., whose results will be dealt with in a separate report. In the total collection of data (Mr B. excluded) the number of correct calls was close to chance expectation, but there was a highly significant displacement effect. The scores on the target cards one ahead (+1 hits) and one behind (-1 hits) the card looked at by the agent were both very significantly below chance expectation (see Table 1).

The totals shown in Table 1 are slightly bigger than those given in the *Journal* for November-December 1951. This is because some additional data received since then has been included. There were also a few score sheets which were excluded from the present assessment because they were so badly written that the number of hits might be questioned. It should be mentioned that there appeared to be no significant deviation from chance expectation of +2 or -2 hits. G. W. F. counted the +2 and -2 hits in a large sample of the data, consisting of the first sixteen runs of all who had completed that number up to the date of the assessment. In 1,184 runs, the -2 hits numbered 5,379, and the +2 hits 5,500, with chance expectation 5,446 and standard deviation 66.

TABLE I
TOTAL DISPLACEMENT SCORES
(Chance expectations in brackets)

Group	No. of subjects	Total no. of runs	- 1 hits ¹	Target hits	+ 1 hits ¹
I	17 subjects completing 48 or more runs each	1224	5694 (5875.2) Dev. = - 181.2	6231 (6120) Dev. = 111	5772 (5875.2) Dev. = - 103.2
II	28 subjects completing 17 to 47 runs each	741	3464 (3556.8) Dev. = - 92.8	3727 (3705) Dev. = 22	3444 (3556.8) Dev. = - 112.8
III	51 subjects completing 16 runs each	816	3843 (3916.8) Dev. = - 73.8	4105 (4080) Dev. = 25	3798 (3,916.8) Dev. = - 118.8
IV	139 subjects completing less than 16 runs each	1078	4865 (5174.4) Dev. = - 309.4	5384 (5390) Dev. = - 6	4966 (5174.4) Dev. = - 208.4
Total for all 235 subjects		3859	17866 (18523.2) Dev. = - 657.2	19447 (19295) Dev. = 152	17980 (18523.2) Dev. = - 543.2
		$t =$ $P =$	- 5.4 10 ⁻⁷	1.2 (insignif.)	- 4.5 10 ⁻⁵

¹ Since in these experiments packs of twenty-five cards containing exactly five of each symbol were used throughout, the probability of a displacement hit, + 1 or - 1, actually varies between 4/24, when there is a direct hit on the corresponding target card, and 5/24 when there is a miss. A good approximation to the average value of the probability of a hit is given by Russell's formula $P = 0.2083 - \frac{0.0416r}{t}$, where r is the total

number of hits and t the total number of trials. In this case the difference is unimportant. $r = 19447$; $t = 25 \times 3859$, which gives $P = 0.1999$ instead of 0.2. This changes the expectation of displacement hits from 18523.2, as given in the table, to 18513.9, a negligible difference which hardly affects the enormous significance of the displacement scores.

The displacement scores were a secondary and unexpected effect, but the deviations were too large to be attributed to chance. The odds against such scores being obtained if chance alone were operating are 100,000 to 1 for the + 1 hits and 10,000,000 to one for the - 1 hits.

FURTHER CONSIDERATION OF THE RESULTS

If the results of these home-testing experiments, considered as a whole, had been more straightforward, they would have been less interesting. An above-chance deviation on the target cards could have been attributed to the informal experimental conditions in which leakage of sensory cues or systematic recording errors might have occurred. Displacement scores, and particularly negative

displacement scores, are not the sort of effect one would expect to result from faulty experimental conditions, and in this case there is the additional complication that many different groups working independently, and not known to each other, contributed to the total result. In fact, the more the Fisk data is examined, the more likely it seems that here is a genuine and quite remarkable ESP effect.

The four sub-groups into which the data has been divided in Table I all contribute substantially to the displacement effect. Group IV contributes the most, and this comprises the 139 subjects

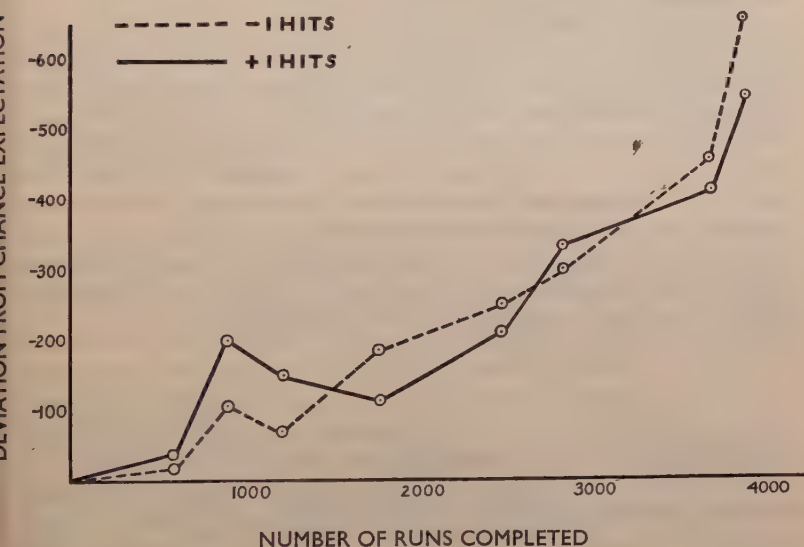


Fig. I

who did less than sixteen runs. This shows clearly that a high proportion of the subjects contributed to the displacement effect, and that it is not due to the results of a few exceptional individuals. Further evidence of the even distribution of the displacement effect through the whole of the data is given by the progress chart (Fig. I) showing the steady accumulation of displacement deviations as more and more data was collected.

One other point which confirms the consistency of the displacement scores is the fact that the observed variance of run scores was close to theoretical expectation. This was worked out by G. W. Fisk on the 3,694 runs collected up to 31 July 1951 (see Table 2).

TABLE 2
VARIANCE OF RUN SCORES (3694 RUNS)

	-1 hits	Target hits	+1 hits
Observed variance	3.90	4.22	3.93
Theoretical variance (npq)	3.84	4.00	3.84

The peculiar nature of the results, negative forward and backward displacement, and its occurrence in a large number of seemingly independent tests led to the suspicion that it was caused by some statistical artefact, possibly something to do with inadequate randomization of the target cards.

That target randomization might in some cases be imperfect was shown when it was discovered, in the course of some analyses to be described later, that the data sent in by one experimenter was deficient in repetitions of the target cards. Scarcely ever did the same card appear more than twice in succession. When this was pointed out to the experimenter, she explained that when she noticed the target card repeated more than once, after she had shuffled the pack, she presumed that this was because the cards had stuck together, and so she separated them! It was decided not to delete this experimenter's data, but if it had been deleted the effect on the results would have been negligible.

TABLE 3

No. of runs in sample	ACTUAL SCORING			CROSS CHECK CONTROL SCORING		
	-1 hits	Target hits	+1 hits	-1 hits	Target hits	+1 hits
607	2711	3041	2715	2902	3037	2908
Chance expect.	2914	3035	2915	2914	3035	2914
Deviation :	-203	6	-199	-12	2	-6
$t =$	-4.20	Insignif.	-4.12	Insignificant		
$P =$	0.00004		0.00005			

To test the hypothesis that the displacement scores were due to a statistical artefact, Fisk acted on a suggestion by Dr Soal and carried out a cross check control on a large sample of the data. In this sample the subjects' calls were scored against the target cards of the immediately succeeding run, as well as against their own targets. Whatever faults were present in the target series would presumably affect both the actual and the cross check scores. Table 3 shows that, whereas the ordinary +1 and -1 displacement scores in the sample tested gave significant negative deviations,

the displacement scores in the cross check were close to chance expectation. The hypothesis of a statistical artefact is therefore improbable.

Simultaneous forward and backward displacement scores with approximately chance-level scoring on the target card has been reported by Soal in the case of the outstanding subject B.S. (5). With B.S., however, the scores were above chance and the scoring level was high. In Fisk's data the displacement scores were below chance expectation and the deviations were small, only becoming statistically significant because of the large quantity of data. The average deviation per run in Fisk's data was -0.17 for the -1 hits and -0.14 for the $+1$ hits. Another point of contrast is that in his 1941 series B.S. produced this forward and backward displacement only when the agent looking at the targets was the same as in the earlier 1936 series. When he worked with other agents B.S. produced either nothing at all or else a forward displacement only. In Fisk's data the simultaneous $+1$ and -1 displacement occurred throughout.

THE 'REINFORCEMENT' EFFECT

In data in which there are both $+1$ and -1 displacement effects, the question arises as to whether the $+1$ and -1 scoring tendencies affect different calls, or whether they operate together on the same call, producing a 'reinforcement' effect. Pratt (3), in his analysis of the double displacement scores of B.S., demonstrated a marked reinforcement effect. When B.S. was getting extra-chance hits on the target cards one ahead and one behind in those trials in which it happened that these target cards were the same, it seemed that the probability of scoring displacement hits was increased. Here are the two different situations in which a call can be made.

Reinforcement Situation
 -1 and $+1$ targets the same

call \swarrow A
 \swarrow B
 \swarrow A

Cancelling-out Situation
 -1 and $+1$ targets different

call \swarrow A
 \swarrow B
 \swarrow C

Dr Soal was the first to note that in reinforcement situations (or 'multiply-determined' calls, as he first named them) B.S. scored significantly better than in cancelling-out situations (5). Bartlett criticised Soal's method of evaluation (1) but Pratt, using two methods, one suggested by Professor H. Robbins and the other by T. N. E. Greville and A. M. Walker, has since demon-

CALL	TARGET	
	+	
+	L	
	O	
L	+	R
	^	
	=	
^	=	
	O	
	=	
=	L	
	L	
	L	
+	^	
	+	
	L	
O	+	
	O	
	^	
=	O	R
	^	
	O	
O	=	
	=	
	+	

Fig. II

strated that Soal was right after all in his conclusions (3). The difficulty in evaluation arose because in any given run the displacement targets of alternate cells overlap, so that the forward and backward displacement scores are not statistically independent. Pratt's method was to abstract non-overlapping target segments of three trials each and to consider only the central call of each segment. The method is wasteful of data, but it is simple to apply. Table 4 is the Robbins evaluation taken from Pratt's paper (3). It shows a definitely higher proportion of hits in the reinforcement situations.

TABLE 4
REINFORCEMENT IN B.S. DATA, SHOWN BY AN
ANALYSIS OF + I HITS AND MISSES
(Chance expectation in brackets)

	Reinforcement Situations	Cancelling-out Situations	162
+ I hits	60 (43.4)	102 (118.6)	290
+ I misses	61 (77.6)	229 (212.4)	452
	121	331	Total

$$\chi^2 = 13.58 \text{ with 1 degree of freedom} \quad P = 0.0002$$

It is a matter of indifference whether + I or - I hits are taken as a basis for the assessment of reinforcement, but they cannot both be considered simultaneously because they are not statistically independent.

D. J. West decided to test the Fisk data for reinforcement by the Robbins method. For this purpose every run was divided into eight consecutive non-overlapping segments of three trials each, the last trial of the run being neglected. The assessment was based on the central call only in each of the eight segments. In effect, each run of twenty-five trials contributed only eight calls to the reinforcement evaluation. It would have been possible to follow Pratt's procedure and abstract twelve independent segments from each run by using the 2nd, 3rd, 6th, 7th, 10th, 11th, 14th, 15th, 18th, 19th, 22nd, and 23rd trials for the reinforcement assessment, but

this was thought to be too complicated for the voluntary workers who helped with the reinforcement counts.

Fig. II is an example of a run divided into eight segments with the two reinforcement situations marked R. For clarity, only the central calls of each segment—that is, the ones used in the assessment—have been written in this illustration. The +1 hits occur in the fifth and sixth segments. In this particular run the distribution of +1 hits and misses is as follows :

	<i>Reinforcement Situations</i>	<i>Cancelling-out Situations</i>	<i>Total</i>
+ 1 hits	0	2	2
+ 1 misses	2	4	6
Total	2	6	8

TABLE 5
ANALYSIS FOR REINFORCEMENT IN +1 HITS
(Chance expectations in brackets)

<i>Group</i>	<i>Total runs</i>	<i>Reinforcement Situations</i>		<i>Cancelling-out Situations</i>		χ^2 (1 deg. fr.)	<i>P</i>
		+1 hits	+1 misses	+1 hits	+1 misses		
I	1224	324 (320.6)	1279	1636 (1637.8)	6553	0.04	—
II	741	185 (200)	815	946 (985.6)	3982	0.26	—
III	816	172 (209.6)	876	1059 (1096)	4421	4.87	0.03
IV	1078	261 (291.6)	1197	1365 (1,433.2)	5801	1.04	0.3
Total	3859	942 (1021.8)	4167	5006 (5152.6)	20757	2.70	0.1
V	530	114 (138.8)	580	667 (709.2)	2879	2.19	0.2

Table 5 shows the distribution of +1 hits in the whole of the data. The data is grouped into the same four sections as were given in Table 1. For each section a 2×2 table has been prepared as already illustrated, and a χ^2 evaluation carried out.

It can be seen from Table 5 that there were 5,109 reinforcement situations and 25,763 cancelling-out situations. The expected proportion in randomly shuffled closed packs of target cards is $1/6 : 5/6$, or $5,145 : 25,727$ in this instance, figures close to what was actually obtained. In this respect, therefore, the targets conformed to the requirements of a random series.

The question arose whether to base the reinforcement assess-

ment on +1 or -1 hits. It was decided to accept the independent suggestion of Dr Pratt and consider the +1 hits.

Table 5 shows that wherever there is an appreciable deviation from chance expectation of +1 hits it is bigger, in proportion to the number of trials, in the reinforcement situations. The differences are insufficient to produce a statistically significant χ^2 except in Group III.

It has already been explained that the method of assessment demanded that two thirds of the data be ignored. The consequence is that in Groups I and II of Table 5 the deviations of +1 hits are not significant. It is therefore unreasonable to expect a significant *reinforcement* of displacement, since the sample in question does not show a significant displacement effect in the first place.

In order to overcome this difficulty a fifth group has been added to Table 5. This consists of data from Groups I and II, that is subjects completing more than 16 runs, but includes only those 13 subjects whose original +1 displacement deviations were greater than or equal to one standard deviation below chance expectation. Table 6 is a 2×2 table prepared from the data of Groups III, IV, and V combined. It shows a significantly greater proportion of misses in the reinforcement as opposed to the cancelling-out situations.

TABLE 6
REINFORCEMENT EFFECT
Groups III, IV, and V Combined

	<i>Reinforcement Situations</i>	<i>Cancelling-out Situations</i>	<i>Total</i>
+1 hits	547	3091	3638
+1 misses	2653	13101	15754
Total	3200	16192	19392

$$\chi^2 = 6.98 \text{ with one degree of freedom} \quad P = 0.008$$

The results given in Table 6 provide evidence that reinforcement was in fact occurring in Fisk's data. After this report had gone to press, a personal communication was received from Dr Pratt pointing out that the Robbins method of evaluating 'reinforcement' of +1 hits fails to take into account the complicating factor of ESP hits on the -1 displacement targets. If there are genuine ESP effects causing both -1 and +1 misses, and these effects are concurrent, then there will be an increased proportion of misses in reinforcement situations, which will not necessarily

be due to an increased effectiveness of individual targets when they happen to be conjoined in reinforcement situations. What the Robbins evaluation does show is that the proportion of +1 hits varies according to whether the -1 target is the same as or different from the +1 target. This seems to imply that the -1 ESP effect and the +1 ESP effect operate together on the same call. It is in this limited sense only that 'reinforcement' has been demonstrated in the Fisk data.

It only remains to be mentioned that two additional analyses were carried out, both with null results. A separate examination of the first two segments of every run was made to see if reinforcement was more marked at the beginning of the run. It was not. A separate examination was also carried out of those reinforcement segments in which the actual target card was the same as the +1 and -1 displacement targets to see if in such situations there was an increase in reinforcement. As is shown in Table 7, there was no significant difference.

TABLE 7

	<i>Reinforcement segments</i>		
	with direct and displacement targets the same, viz <i>A</i> → <i>A</i> <i>A</i>	with direct and displacement targets different, viz <i>A</i> → <i>B</i> <i>A</i>	
Displacement Hits	136	806	942
Displacement Misses	642	3525	4167
Total	778	4331	5109

$$\chi^2 = 0.1 \text{ with one degree of freedom} \quad P \approx 0.7$$

DISCUSSION OF THE FINDINGS

In casting about for an explanation of the puzzling effects in Fisk's data, one fact stands out. The numerous subjects who contributed to the same odd and unexpected effect had only one thing in common, their relationship to G. W. Fisk, the organizer of the project.

It is no new suggestion that the experimenter in an ESP test is to some extent responsible for success or failure, but the implication of these results is more than that. If G. W. Fisk was in truth the cause of the ESP effects in the data which he so diligently collected, then it would follow that an organizing experimenter, even though he was not present in person during the tests,

can govern the type of response produced by many different subjects.

This hypothesis is to some extent supported by the observation that various well-known workers in the field of ESP research have tended to collect results peculiar to themselves. J. B. Rhine found above-chance scoring on the target card a relatively common occurrence, and that it made no essential difference whether there was an agent looking at the target cards. S. G. Soal has found consistent above-chance scoring only in exceptional subjects and in them only when there was an agent looking at the target cards. W. Whately Carington, using drawings exposed at the rate of one per day, and conducting mass tests with unselected subjects, reported above-chance scores and a temporal displacement of ESP effect on to the targets of adjacent days, but no other experimenter, before or since, has obtained comparable results. B. M. Humphrey again in tests with unselected subjects, has reported positive and negative scoring tendencies correlated with personality traits, but attempts at repetition by other experimenters have produced different results. Denys Parsons and D. J. West belong to a group of experimenters who seem to collect consistently null results under all circumstances. G. W. Fisk seems to be a particularly successful experimenter, for he has recently obtained interesting results with his new clock cards and also in some PK tests. The only experiment which has so far been conducted jointly by D. J. West and G. W. Fisk produced null results (2), but others are in progress. Independent tests by different experimenters on the same group of subjects is the obvious line of investigation. We may well find that the experimenter is the most important factor in determining psi success.

Shifting responsibility for the peculiar results from subjects to experimenter does not, of course, explain the peculiarities. J. B. Rhine (4) has shown that changes in attitude and motivation do not provide an adequate explanation of all the odd ESP responses that have been reported. He favours the hypothesis that distorted responses are due to systematic error independent of changing motivation, rather like a man inadvertently shooting always to one side of the bull's-eye. Unlike the marksman, the psi subject cannot correct his error, because the ESP process is unconscious. If Fisk's negative forward and backward displacement is due to systematic error, then it would seem to be Fisk who determines, albeit unconsciously, the fact that an error is made. One must in the end agree with Rhine that 'the operation of psi missing is not yet understood'.

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ICHTHYOSIS TREATED BY HYPNOSIS

ABSTRACT OF CORRESPONDENCE IN THE 'BRITISH MEDICAL JOURNAL'

Dr A. A. Mason's report of a case of congenital ichthyosiform erythrodermia of Brocq treated by hypnosis, which was summarised in the last number of this journal, has been the cause of some controversial correspondence in the *British Medical Journal*.

13 September 1952. Dr John Freeman writes stating that he unhesitatingly classifies ichthyosis as one of the allergic disorders, such as hay fever and asthma, which are affected by emotions, so that a cure or amelioration by hypnosis should cause little surprise. None the less, Dr Freeman finds it strange that the improvement should have been localised according to the suggestions of the hypnotist, and he raises the question of how accurate was the localisation.

27 September 1952. Dr Kathryn H. Cohen, who observed the treatment of the case over a period of months, thinks it no help to apply the loose term allergy to ichthyosis. She asks if Dr Freeman, as an allergist, has discovered any means of relieving this distressing condition.

11 October 1952. In reply to Dr Freeman, Dr Mason writes: 'The improvement in the case of ichthyosis reported was strictly area by area in accord with suggestion made under the hypnotic trance—namely, first, the left arm; secondly, the right arm; and, thirdly, trunk and legs. The response was strictly local, and at one time one arm was totally clear while the rest of the body was completely unaffected.' Dr Mason asks what is the evidence that

ichthyosis is an allergic disorder. Has the causative allergen been recognized and have patients been relieved by de-sensitisation? He also asks for details of cases of ichthyosis that have been affected by emotions.

In reply, Dr Freeman maintains that there is adequate evidence that the ichthyosis is an allergic disorder, although the allergen cannot be produced. Ichthyosis is something of a rarity in general skin clinics, but it is found in its various stages to be very common indeed among the asthma, hay fever, and migraine cases at an allergy clinic. Dr Freeman points out that knowing the cause does not necessarily lead to cures, but he gives references to reports on the effect of emotions upon allergic disorders.

1 November 1952. The correspondence is concluded by a letter from Dr F. Ray Bettley pointing out that Dr Freeman appears to be referring to the comparatively common ichthyosis simplex, which is often associated with the atopic eczema-asthma-hay-fever syndrome, whereas Dr Mason's case, which was demonstrated at the Royal Society of Medicine, was one of erythrodermia ichthyosiforme, a congenital disorder in which structural abnormality is more important than functional deviation. 'It is surprising that it should respond to any kind of treatment; that it should respond to hypnotic suggestion demands a revision of current concepts of the relation between mind and body.'

REVIEWS

NATURERKLÄRUNG UND PSYCHE. By C. G. Jung and W. Pauli. Zurich, Rascher Verlag, 1952. (Studien aus dem C. G. Jung Institut.) 194 pp.

Both parts of this book are of great interest, but in quite different ways. I shall first say a word or two about the second part, Professor Pauli's essay on Kepler. This is a very scholarly and illuminating account of an important phase in the history of science, and the author is himself a distinguished physicist. He tries to show how some of the fundamental concepts of mathematical physics gradually emerged in Kepler's mind out of a background of emotionally-charged 'archetypal' ideas, and never wholly freed themselves from this background. For example, Kepler's mathematical physics was closely connected with his Christian Platonist theology or theological imagery, which led him to interpret the

doctrine of the Trinity in terms of geometrical analogies. Professor Pauli illustrates his argument by a well-documented and very various account of the controversy between Kepler and his contemporary, the mystically-minded alchemist Robert Fludd of Oxford. If this *Journal* had been a journal of the history of ideas, Professor Pauli's essay would have required a very careful analysis, by someone much more learned than the present reviewer. I think, however, that it has little direct relevance to psychical research, and I shall say no more about it here.

Professor Jung's essay, on the other hand, will have to be read and pondered over by all serious students of our subject, and it should certainly be translated into English as soon as possible, though it must be confessed that the translator will not have a very easy task. For what Professor Jung is doing is to propose a new theoretical framework into which all sorts of *prima facie* supernormal phenomena may be fitted. He proposes it in a somewhat tentative manner. He tells us that he hesitated for a long time before publishing his thoughts on the subject, though they have occupied his mind pretty continuously for many years. But he claims to have collected enough evidence, both from his own clinical experience and from the literature of psychical research, to show that his theory is at any rate worth serious discussion.

We usually assume that supernormal occurrences must have a causal explanation, if only we could find it. For example, we ask what is the 'modus operandi' of supernormal cognition, or of PK. According to Professor Jung, that is the wrong question to ask, and therefore we ought not to be surprised if we fail to find a satisfactory answer; indeed, it is even *unthinkable* that there should be a causal explanation of such happenings. For one of the characteristic features of psi phenomena is their independence, or partial independence, of Space and Time. In such occurrences, Space and Time are 'elastic' in relation to the psyche (pp. 19-20) and this by itself is enough to show that the notion of Causality can have no application to them. A causal transaction, according to Professor Jung, is always an 'energetic' phenomenon (in the physicist's sense of the word 'energy'.) The concept of Cause, then, can only be applicable to the world of moving bodies, or rather, to the world of *macroscopic* moving bodies.

It follows from this, of course, that even the notion of 'normal' mind-body interaction is in the end unthinkable, as some of the rationalistic philosophers of the seventeenth century maintained. Even Epiphenomenalism would be an absurd hypothesis, because it does involve a causal relation between body and mind, though only a unilateral one. The only 'thinkable' theory of the relation

of mind and body would be one form or another of Psychophysical Parallelism. I shall return to this point later.

For the moment, however, we are only concerned with psi phenomena. If we hold, as Professor Jung does, that no causal explanation of them is even conceivable, we are confronted with a dilemma. Either we must write them off as mere chance coincidences; and this, Professor Jung thinks, we cannot reasonably do, especially when we consider the results of Professor Rhine's work. Or else, on the other hand, we must devise some new, and *non-causal*, type of explanation for them. This is what Professor Jung tries to do. He suggests that there are two mutually irreducible types of order in the universe: the familiar causal sort of order, and another quite different sort of order (at right angles, as it were, to the causal one) which has to be defined in terms of 'synchronicity'. Synchronicity, he explains, is not just synchronousness. In a 'synchronicity phenomenon', as he uses the phrase, two contemporaneous events are linked together in a meaningful manner (*sinngemäß*). Such a phenomenon is, if you like, a coincidence, because there is not, and indeed cannot be, any causal explanation for the contemporaneousness, even though there is one for each of the events taken separately. But a synchronicity phenomenon is not a *mere* coincidence. It is a coincidence which 'makes sense', which is somehow 'meaningful'. (I am afraid there is no exact equivalent in English for the German word *Sinn*, which plays a crucial part in Professor Jung's argument.)

Professor Jung remarks that such meaningful coincidences often have a numinous character for those who experience them. By this I suppose he means that religious people would regard them as providential and non-religious people as uncanny. Curiously enough, the concept of 'Providence' is not mentioned anywhere in this essay though it seems to be highly relevant to Professor Jung's theme. However this may be, he explains the numinous character of synchronicity phenomena by supposing that 'archetypes'—archetypal ideas belonging to the Collective Unconscious—somehow manifest themselves in such occurrences. And certainly our first inclination, however much we may disavow it later, is to say that such a numinous conjunction of events cannot be just a coincidence.

I will now mention one or two of Professor Jung's examples. On a certain day he had fish for lunch. In the morning, he had made a note of an inscription 'est homo totus medius *piscis* ab imo'. In the afternoon, a former patient, whom he had not seen for months, showed him some very striking fish-pictures which she had painted. In the evening, he is shown a piece of embroidery

depicting fish-like sea monsters. The next morning, another patient recounts a dream about a large fish. All this occurred at a time when he himself was studying the historic Fish-symbol, though none of the people concerned knew that he was doing so. Again, the astronomer and psychical researcher Flammarion was writing a book on the atmosphere ; and just when he had got to the chapter on the strength of the wind, a sudden and very strong gust of wind blew all his loose papers out of the window! Professor Jung mentions a number of other 'spontaneous cases' in the course of his essay, many of them drawn from his own clinical experience. Most of them are too complicated to quote here, but I will briefly summarise two. A patient, at a very critical stage of her treatment, was recounting a dream about a golden scarabaeus. Suddenly there was a noise. An insect had flown against the window. It was not, of course, a golden scarabaeus ; but it was a kindred species of beetle, 'the nearest analogy to a golden scarabaeus which our latitudes can produce' (p. 22). In another case, the wife of a patient recounts that when her mother died, and also when her grandmother died, a large flock of birds appeared outside the house. Unknown to her, Professor Jung sends her husband to a heart-specialist. Soon after her husband has started, she sees a large flock of birds alight on the house. This fills her with anxiety. On his way home, the husband collapses in the street, and dies very shortly afterwards (p. 23).

Of course we are inclined to say that such 'meaningful' coincidences *are* only coincidences after all, however numinous they may be for those who experience them. If they were something more than mere coincidences, surely they ought to occur with a frequency exceeding the chance expectation? But at this point Professor Jung invites us to compare these spontaneous cases with the analogous experimental cases. In the experimental cases, for instance in the work of Dr Rhine, we find that such 'meaningful' collocations of events do occur with a frequency significantly exceeding the chance expectation. Thus there really is something which needs explanation, both in the experimental cases and in the spontaneous ones. If a causal explanation is 'unthinkable', on the grounds already mentioned, we must find another. And the only one available is the hypothesis that there is another principle of order in the universe, over and above the principle of Causality : namely the principle of non-causal Synchronicity, where two or more synchronous events go together in a sense-making or meaningful manner.

Of course this is not a wholly new idea. As Professor Jung emphasises, it or something like it is a very old one, though in the

last century and a half it has come to be forgotten, at any rate among educated Western Europeans. Many ancient methods of divination seemed to be based on some such theory. The case of the flock of birds cannot fail to remind us of the methods practised by the ancient Roman augurs. (An ornithologically-minded reader cannot but regret that Professor Jung has so little to say of these respectable bird-watching persons.) Again, in the middle ages there was the queer metaphysical theory of the *signatura rerum* ; and in Leibniz's doctrine of the Pre-established Harmony, as Professor Jung points out, something like the Principle of Synchronicity actually replaces the Principle of Causality, at any rate so far as the created universe is concerned. A student of psychical research, if he happens to read Leibniz's *Monadology*, will naturally be inclined to interpret the Pre-established Harmony as a kind of telepathy, extended to a cosmic scale ; and it may occur to him that this is not, perhaps, the only case in which queer metaphysical speculations have anticipated future empirical discoveries. It would appear too that every Leibnizian monad has a precognition of its own later experiences.

Moreover, it is obviously tempting to interpret the beliefs and practices of Astrology in terms of the Principle of Synchronicity. Accordingly, Professor Jung undertook what he calls 'an astrological experiment', which is described in Chapter 2 of his essay. I confess that there is a good deal in this chapter which is far too technical for me to understand. But very roughly, the question which the experiment was designed to answer was this : when we compare the horoscopes of married persons with those of unmarried ones, do we find statistically-significant differences? For one (fairly large) sub-group of cases, it seemed that there actually were such statistically-significant differences. But when *all* the cases which Professor Jung had collected were considered together, the result did not differ significantly from the chance expectation. He concludes that from a purely scientific point of view there is little hope of establishing any kind of 'lawful' correlation here. Nevertheless, the fact remains that there was an above-chance result in quite a large sub-group of cases considered. And this, he says, must be regarded as a Synchronicity phenomenon. If I follow him rightly (I am not sure that I do) his view may be crudely expressed thus : on the whole Astrology doesn't work, but to the (limited) extent that it does, we must suppose that there is a non-causal 'connivance' between the objective facts and the psychical state of the astrologer. Such a 'meaningful' correspondence between one's psychical state and observed environmental events is the same kind of Synchronicity phenomenon as we

noticed in the case of the scarabaeus above, or in the case of the body and the flock of birds.

We may now return to the relation between mind and body. As we have seen, it follows from Professor Jung's very strict notion of causality that there cannot be any sort of causal relation between mind and body. If he is right, some form of Psycho-physical parallelism is the only possible theory. Now should the parallelism of mental and bodily processes in a given human individual be regarded as a Synchronicity phenomenon, a meaningful but non-causal correspondence between two mutually independent series of events? Professor Jung appears to answer 'Yes'. I confess that I find this part of his discussion exceedingly difficult. But if it is obscure, there is at least some excuse for the obscurity. Paralelistically-minded thinkers have not hitherto discussed the bearing of 'out-of-the-body' experiences on their theory; and if they had, they would obviously have involved themselves in considerable perplexities. Professor Jung does discuss out-of-the-body experiences, and gives a full description of a very interesting one which occurred to a patient of his own immediately after a surgical operation (pp. 93-4). Professor Jung asks whether such an experience is to be counted as a Synchronicity phenomenon, especially when verifiable information is acquired about environmental events, as it was in this case. After a long and very intricate discussion, he appears to answer 'No', on the ground that such experiences may still have a physiological 'substrate', namely the sympathetic nervous system, even though the brain is temporarily out of action. This is puzzling, because he appeared to maintain earlier that all correlations between mental and physiological events in the same person *are* Synchronicity phenomena. Perhaps what he ought to have said is this: an out-of-the-body experience must in any case be a Synchronicity phenomenon of some kind, because that is what all the experiences of a living human being are. The question is what sort of Synchronicity phenomenon is it? Is it just a special case, though a very uncommon one, of the 'normal' mind-body synchronicity; or is it, so to speak, a *direct* synchronicity between mental events and *environmental* happenings without any physiological mediation? If that is Professor Jung's question, he is deciding, after some hesitation, for the first alternative. It would follow, of course, that out-of-the-body experiences cannot be regarded as evidence in favour of survival. They would have no tendency to show that discarnate mental activity is possible, as some people think they have. Indeed, it seems that Professor Jung's new 'synchronistical' version of Psycho-physical Parallelism, like the earlier versions of the same doctrine, would exclude

the possibility of survival altogether, though he does not himself discuss the question in this essay.

I have now given a brief account of Professor Jung's Principle of Non-causal Synchronicity, so far as I understand it. Will it do what he claims for it? Does it provide us with the theoretical framework which we need for making supernormal occurrences intelligible? Perhaps it is premature to try to answer this question at present. An idea so strange, and (to modern European readers at least) so revolutionary, is likely to need much discussion and clarification before we can decide whether or not it is acceptable. We cannot expect that when it first appears in the world, or reappears after a long period of eclipse, it will be formulated in an easily understood or even a wholly self-consistent way. And with all respect to Professor Jung, I cannot think that he *has* formulated it in a wholly intelligible manner. I shall now mention one or two difficulties which are likely to occur to the reader.

First, the very term 'Synchronicity' does not seem altogether suitable for Professor Jung's purpose. The whole point of precognition is that the event precognized is *not* contemporaneous with the mental event which is correlated with it. Indeed, if it is a characteristic mark of psi-phenomena in general that Time, as well as Space, is 'elastic in relation to the psyche', a temporal word like 'synchronicity' would seem to be just the wrong one to use. It would have been better, perhaps, to speak just of a non-causal but meaningful *correspondence* between events.

Secondly, Professor Jung appears to say sometimes that *both* the events in a synchronistical pair have to be mental events, though at other times he appears to say that there can be a 'meaningful' correspondence between a mental event and a purely environmental one. I do not know which of these two views he wishes to maintain. But it is worth while to point out that the 'bi-mental' view, if one may call it so, involves us in considerable difficulties. Sometimes, of course, it does fit the facts well enough. Thus in the Flock of Birds case, Professor Jung is able to argue very plausibly that the relation of 'meaningful' correspondence is a relation between the wife's psychical state of anxiety about her husband and her *visual experience* of seeing the flock of birds; thus both the 'synchronistically' related events were mental ones. It is true that she was not consciously anxious about her husband, and did not even suspect (consciously) that he was suffering from heart-disease. But Professor Jung thinks it possible, and indeed likely, that unconsciously she not only suspected this, but knew it; 'her unconscious', he thinks, knew it, though 'her conscious-

ness' did not. The visual experience of seeing the birds, an event already associated, for her, with deaths in the family, reminded her of this unconscious knowledge; and consequently this visual experience acquired for her a numinous, and ominous, quality. (We may notice in passing that there is now a *causal* factor in the explanation; associative reminding is a causal process.) But suppose we consider the familiar sort of ESP experiment, in which the card-guessing method is used, and let us suppose that the results are positive—the percipient scores significantly above the chance expectation in a sufficiently large number of trials. This too, according to Professor Jung, is a case of non-causal but 'meaningful' synchronicity. But what is synchronous with what? With regard to the percipient's half of the Synchronicity phenomenon Professor Jung's answer is fairly clear. The percipient has an unconscious knowledge of the target, and this knowledge is released, so to speak, or enabled to manifest itself in consciousness, by his emotional attitude towards the experiment. But what about the other half of the phenomenon, that with which the percipient's state of consciousness 'non-causally' corresponds? In telepathy, it is another mental state. But it is a mental state in another mind, whereas in the Flock of Birds case both the (unconscious) anxiety and the perception were in the same mind. Perhaps this does not matter. But what are we to say of pure clairvoyance? And what are we to say of PK? It is at least conceivable that PK might occur when neither the agent nor anyone else perceives the environmental event which is 'synchronistic' with the agent's volition. Or shall we say that either he or someone else does always perceive it, consciously or unconsciously, by extrasensory perception if not by ordinary sense-perception? I can see no evidence for saying so; and I conclude that the 'bi-mental' interpretation of Synchronicity is too narrow to cover all the facts, though it certainly covers some of them.

Finally, we must notice that Professor Jung's whole argument depends upon his very narrow and rigid conception of causality. But why should we accept this conception? Professor Jung writes as if Hume had never existed. Like the Rationalist philosophers of the seventeenth century, he seems to think that there ought to be some sort of rationally-evident connection between cause and effect. It follows that even where we have abundant empirical evidence for the existence of a causal relation between two events, we may nevertheless be obliged to deny that there is any causal relation between them, on the ground that it is 'unthinkable'. Thus in telepathy, for example, it is 'unthinkable' that there should be causal relation between an event in A's mind and an event in B's

mind, however strong the empirical evidence is. Those who have been brought up in the Anglo-Saxon Empiricist tradition will be disposed to object that neither 'necessities of thought' nor 'un-thinkabilities' have much relevance to a discussion of causality. They will say that the question whether there is or is not a causal relation between X and Y is a purely empirical one. If we can establish empirically that there is a reliable correlation between the two, then there is *ipso facto* a causal relation between them, however surprising it may be. We cannot lay down beforehand, on *a priori* grounds, what causal relations are possible in the universe and what are not. Nor can we know *a priori* that if telepathy, for instance, be a causal transaction there 'must necessarily be' an intermediate causal chain of some sort between events in the agent's mind and events in the percipient's mind. For all we can tell *a priori* there might be none; telepathy might be a *direct* causal relation between mind and mind, and equally PK might be a direct causal relation between mind and matter. It is purely a question for empirical research. In any case, there must be direct causal relations *somewhere*. If there were not, the language of 'intermediate links' would be pointless, for how would the 'links' themselves be connected?

Professor Jung himself says (p. 68) that when we are trying to explain supernormal occurrences, we have a choice between two alternatives, non-causal synchronicity on the one hand, and what he calls 'magical causality' on the other. If this is the choice, I think that empirically-minded persons will prefer 'magical causality', though they may fairly complain that the adjective 'magical' has a somewhat dyslogistic flavour, and they may think it would be better to use the emotionally-neutral prefix 'psi-'.

Some Empiricist thinkers, however, would go further. They would ask whether there is any empirically-detectable difference between the consequences which might be expected to follow if Professor Jung's non-causal Synchronistic theory of supernormal phenomena were true, and those which might be expected to follow if the theory of 'magical causality' were true. If the answer to this question is 'No', these thinkers would maintain that we are merely being invited to choose between two alternative *terminologies*. Do we prefer to describe the admitted facts in a causal language, or in a non-causal synchronistic language? We may use either, they would say, though we must not of course use both at the same time. In the words of Professor John Wisdom, 'Choose which you like, but be careful.'

But perhaps we ought to hesitate a little before we accept such a deflationary conclusion. It is conceivable that there might after

It will be an empirically-detectable difference between the consequences of the two theories some day, even though not at present. If Professor Jung's theory were right, it would follow, I think, that there never could be such a thing as *applied* psychical research (in the sense in which the technology of aircraft construction can be called 'applied physics'.) But if the processes which psychical researchers study are causal ones, it should be possible some day to subject them to voluntary control. For example, if there is a causal explanation for apparitions, we can hope some day to find out 'how they work', and then we should be able to produce apparitions voluntarily. But if apparitions are non-causal and purely 'synchronistic' phenomena, as Professor Jung thinks they are, it would seem that we can have no such hope. If apparitions are non-causal phenomena, the notion of 'producing' can have no application to them, and the question of 'how they work' can have no application either. Thus in the future, though not yet, it is possible that Professor Jung's theory might be empirically falsified. On the other hand, I do not see how it could ever be empirically verified. In the future, as at present, the partisans of 'magical causality' will always be able to argue that there is a causal explanation for supernormal phenomena, even though they have not yet succeeded in finding out what it is.

H. H. PRICE

DET OCKULTA PROBLEMET. By John Björkhem. Uppsala, J. A. Lindblads Förlag, 1951. 194 pp. Kr. 8.50.

Dr Björkhem is a man of great learning and wide experience. He has studied and taken degrees in theology, in philosophy, and in medicine, and he now practises as a psycho-therapist in Stockholm. He has published an extremely interesting book, entitled *De hypnotiska Hallucinationerna*, which is based on his own hypnotic experiments in Uppsala and was submitted as an academic dissertation at Lund in 1942.

The present work is more popular in character. It first appeared in 1939, and this revised and enlarged second edition is dated 1951. The book is divided into two Parts of very unequal length. The first, containing 118 pages, begins and ends with two chapters of a general nature, 'Parapsychology and Science' and 'The Possibilities of Explanation in Parapsychology'. Between these come a sequence of seven chapters treating the following special topics, viz., telepathy and clairvoyance, psychometry, psychic healing, hypnotism and crime, English spiritualism, automatic writing and 'speaking with tongues', and telekinesis and materialization. The

second Part, containing twenty-eight pages, describes two cases with which Dr Björkhem was personally concerned. One relates to the circumstances of an alleged murder at Esarp in Skåne, which excited a good deal of interest in Sweden at the time of the trial of the accused and later when he successfully appealed. The other is a very queer story indeed, concerning an alleged series of appearances of the late Mr Harry Price to a patient in the hospital at Lund at which Dr Björkhem was acting as house-physician.

Dr Björkhem has a very wide knowledge of the literature of psychical research and of spiritualism, so the various chapters in Part I provide an excellent synoptic view of the subject. As regards this Part, I will confine myself to the following scattered remarks.

(1) Dr Björkhem states that between the years 1930 and 1950 he has been responsible for rather more than 30,000 hypnotic experiments on rather more than 3,000 subjects, mostly students in Uppsala or Lund. He claims to have found repeated evidence for telepathy and 'travelling clairvoyance' in these experiments, and he gives examples (p. 42). (2) He states that an hypnotic hallucination which he suggested to a subject in 1934 (viz., that the subject should see his own portrait on what was in fact a certain blank card) was still persisting in full strength as late as 1946 (p. 75). (3) The conclusion of his discussion on the possibility of inducing criminal actions by hypnosis is that a subject can be forced by hypnotic influence to do anything that he could be compelled by force of circumstances to do in the waking state, but nothing further (p. 87). (4) Dr Björkhem relates that in 1937, when he was on a visit to London, he had an anonymous sitting with Mrs Dowden. In the course of the sitting he asked, 'Who will be the new bishop of W.?', thinking of the diocese of Wäxjö which was then vacant. Mrs Dowden's hand wrote the word *Brilioth*. Some months later Professor Brilioth, as he then was, was appointed to the bishopric (p. 105). (5) Dr Björkhem takes a very cautious attitude towards alleged physical phenomena, and concludes that in the last resort the question can be decided only by mechanical and electrical methods of control and recording (p. 132). (6) As regards mental phenomena his conclusion is much more positive. 'Those who will not accept the existence of telepathy, clairvoyance, and psychometry ought at once to join the spiritualists; for in that case there remains no other explanation but the hypothesis of spirits. The hypothesis of fraud will go far in many cases, but it is not capable of explaining all the observed phenomena' (p. 137).

I will conclude with a very brief description of the two cases which constitute Part II. One's estimate of their value would of

course depend on a full account of the details, and this cannot be given in a review.

(1) *The Esarp case*. On February 2nd, 1932, a middle-aged woman called Hanna Anderson was found dead in the mill-pond at Esarp near Lund. Her husband, Nils Anderson, who had been on bad terms with her, was suspected of having murdered her. He was tried and convicted and given a life sentence. (There is no capital punishment in Sweden). Many years afterwards an appeal was made for a renewed investigation of the facts and the annulment of the sentence. This was successful. The motion for a new trial was accepted on October 21st, 1947, and in December 1950, Anderson, who had been set free, was awarded 150,000 kronor in compensation for wrongful imprisonment. (I believe that I am right in saying that the income tax authorities promptly tried to tax this as a 'windfall'!)

On November 22nd, 1945, Dr Björkhem handed to a hypnotic subject in Malmö six envelopes, each containing an object of different *provenance*. In one of them was a photograph of Hanna Anderson taken shortly after the discovery of her corpse and portraying her in the clothes in which she was found. The subject chose this envelope as the only one which gave her any impressions. It was slit open along one edge so that she could insert her hand and feel the photograph, but she did not take it out or look at it.

The subject proceeded to talk as if she were present at the scene of the tragedy. She stated correctly that the time was *winter*; that it happened near a *mill* in the *country*; that it concerned *a man, his wife, and his mistress*; that the man had to do with *selling grain*; that his name was *Nils* or *Johan* or *Jöns*; that he was a *dissipated* person who was *often out on the loose* and *did not care for his wife*, who was *older* than he and *careless* in her dress and person. She sees the woman lying *fully clothed* in the water *beside a jetty*. Near her she sees an object which she describes as a *leg bound with metal*. (It was in fact a coffee pot.) She senses that a *post-mortem* has been performed on the body, getting a visual impression of *men in white coats* and a characteristic olfactory impression. It is not clear to her how the woman met her death; she sees that the man is *suspected and examined*, but is sure that he did *not murder her*. She sees *scars* on the woman's body.

Dr Björkhem states categorically that he answered none of the subject's questions and gave her no help or indication of whether he was right or wrong in the course of her statements.

(2) *The Harry Price case*. On October 8th, 1948, a Swedish textile worker, whom we will call *E*, was admitted at his own urgent

request to the hospital at *Lund*, in spite of the fact that he belonged to another district, suffering from abnormal emaciation. Dr Björkhem was acting as physician in the department of the hospital to which *E* was assigned. On the following day *E* asked for a private conversation with Dr Björkhem, who was a complete stranger to him. In this interview he made the following extraordinary statement. At the end of March or the beginning of April 1948 he had awoken in the night to see an unknown elderly gentleman standing beside his bed. This gentleman had talked to him in a friendly way, but in a foreign language which he did not understand but took to be English. He gathered, however, that the gentleman was called *Price*, and got the impression that he was a *doctor or an engineer*. The figure then suddenly vanished. Since then 'Price' had frequently appeared to *E*, who had gradually learned a little English and begun to understand what 'Price' said to him. 'Price' stated that he had *recently died*, and that he had *devoted much of his life to psychical investigation*. (Mr Harry Price in fact died on March 29th, 1948. Dr Björkhem was unaware of the fact at the time, and so far as he knows no account of Mr Price's life or death appeared in any Swedish paper.) *E* had attempted to photograph 'Price', without success and to the latter's considerable amusement. 'Price' had taken a friendly interest in *E*'s affairs, and had given him frequent advice. It was he who had insisted that *E* should go to the *Lund* hospital for treatment of his emaciation. During the first night at the hospital 'Price' had again appeared to *E*, had told him that he had come to the right place, and had earnestly requested him to confide his experiences to the doctor who had investigated him (viz., Dr Björkhem) and to no other. *E* asked Dr Björkhem whether he knew of any English psychical researcher of the name of 'Price' but Dr Björkhem gave him no information.

I shall not pursue this story further into the rather complicated medical and psychiatric developments which Dr Björkhem relates. Enough of it has been given to indicate its extreme queerness, and, like Dr Björkhem himself, I do not know what to make of it. It just confirms the platitude that the world is a very odd place and that there are very odd people and events in it.

C. D. BROAD

THE DEVILS OF LOUDUN. By Aldous Huxley. London, Chatto & Windus, 1952. 376 pp. Illus. 18s.

Although the story of the outbreak of diabolic possession among the Ursulines at Loudun in the seventeenth century is well

known to most students of the subject, this is the first time that it has been told in detail in English and in a form acceptable to the general reader. It is a pathetic story of the ignorance, credulity, and social maladjustment among Roman Catholic circles of the time, and throws a lurid light upon the state of those who, immured in conventual establishments, could only find an outlet for their natural emotions and desires in ways so terrifying that diabolic possession was believed by some to be the only way to account for them.

The story of the possessed nuns revolves around the figure of Urbain Grandier, the fascinating Loudun priest, whose appearance was such that many a woman felt her knees give way when her eyes alighted on him. His conquests furnished the theme of much malicious talk, hatred, and jealousy, and as the story unfolds the net is seen gradually being drawn more and more tightly around him until finally nothing but the torture chamber and the stake await him.

In the course of his reconstruction of the events* at Loudun Mr Huxley draws a vivid picture of the conditions obtaining at the time. He describes the paroxysms of lust which convulsed the nuns, the harm done by the continual exorcisms in which the women became exhibits before crowds of gaping tourists, the futile attempts to weaken the credulity of the clergy, and the general atmosphere of hatred, intrigue, hypocrisy, and revenge which overshadowed the melancholy scene. Had Mr Huxley been content to give a straightforward history of the Loudun affair the book would have been one in which it would have been hard to find any fault. As it is, however, the story is marred by the introduction of expressions and modes of speech reminiscent of Hollywood, of irrelevant material doubtless of importance to the author and to the few readers who share certain of his peculiar interests, and a good deal of psychological discussion which would have found a better home within the covers of an American digest of popular science. Apart from these blemishes the book is a fascinating introduction to one of the most famous cases of possession on record, and the list of authorities is well selected, although Mr Huxley, when dealing with the contribution of Jacques de Nyau (or Niau), insists throughout on spelling it Nion.

E. J. DINGWALL

CORRESPONDENCE

TRAINING FOR RESEARCH IN PARAPSYCHOLOGY

SIR,—In preparation for conventional research the experimenter undergoes years of training, during which he unconsciously acquires a feeling for his subject. This process of 'preparation by immersion' is universal throughout science, but in established fields of research it is carried out automatically by the educational system, and the tyro is easily discovered by the way he mishandles standard techniques and ignores well-established facts. The situation in a new field, like parapsychology, is different. There are few facts and no proven techniques. Everyone is equally qualified to propose and to conduct research—or at least such is the fairminded assumption that the scientist from another field might make. The fallacy of this assumption is two-fold in that it concerns both the nature of the field and the nature of present knowledge in the field.

Because the experimental facts are few, it is easy to assume that the beginning problems must be simple. The point of view that needs emphasis is that psi phenomena are *more*, and not *less*, difficult than the other problems in modern physics. We are dealing with phenomena that lie beyond the present frontier of knowledge, so that to expect easy answers is unrealistic.

In most cases these phenomena have appeared only as marginal and evanescent effects in a thoroughly worked-over region of human experience. This is not a scientific treasure-trove upon which we have fortunately stumbled and which needs only to be shovelled into a convenient basket. Rather, we face a refining project involving the tailings of all past scientific endeavour. If there is something of value to be extracted, it will require subtlety, persistence, and the combined effort of many workers.

The fallacy that everyone is equally prepared for research in parapsychology rests secondly upon a misunderstanding of the nature and extent of present knowledge in the field. For this perhaps the people within the field should share the blame. A review written for the non-specialist states the experimental results as simply as possible, slighting the difficulties, and omitting the failures except in so far as these affect the total statistical picture. The reader who is not yet convinced of the reality of the effects has no interest in speculative psychological relationships that do not bear upon the proof-of-occurrence, however important they may be for the successful evocation of the phenomena. Thus, the average scientist pictures these phenomena as occurring in a semi-

magical situation in which the experimenter goes through certain mechanical motions and the phenomena either happen or they don't. Small wonder that these phenomena, when so conceived, are hard to accept into one's scheme of things.

The truth is that the laboratory experimenter works very hard. He draws upon all that he has learned from past experience, striving to motivate and yet relax his subjects so that these unusual psychic effects will occur. The successful experimenter has been likened to an artist playing upon his subjects as upon a musical instrument. He needs every bit of understanding he can muster (and perhaps, also, certain as yet undefined personality characteristics). Immersion in the past art is of great importance, all the more so since little explicit factual information is available.

In circumstances like these no one is qualified to guide another. Every experienced worker must be given a free hand. It goes without saying that most of the effort will be unsuccessful. There will be just as many foolish ideas suggested as in conventional fields, but no way exists to weed them out. The intuition of genius is called for. Meanwhile, progress will be slow and expensive.

R. A. McCONNELL

Department of Physics,
University of Pittsburgh.

MR RICHMOND'S EXPERIMENTS ON PARAMECIA : INTERPRETATION OF THE RESULTS

SIR,—Experimenters must often find the arm-chair critic as irritating as any back-seat driver, so I hope I may be forgiven if I offer a critical comment on the impressive results recently reported by Mr Nigel Richmond ('Two Series of PK Tests on Paramecia' in the *Journal*, March-April 1952). If we consider whether the results could be attributed to ESP instead of PK, the method leaves open a loophole. It seems possible to explain the results by supposing (a) that Mr Richmond *qua* subject was precognizing the future movements of the paramecia, and (b) that, when he cut by hand a pack of cards to determine into which of the quadrants he should 'will' the paramecia to swim, he was guided by clairvoyance in selecting a card of the appropriate suit. This loophole could be closed, e.g. if the 'target' (the alternative which the subject is to 'will') were determined mechanically by a random selector. I hope that this experiment will be repeated, using such a modified technique. The conclusion suggested by Richmond's results—that organisms are susceptible to PK influence to a much

greater extent than inorganic bodies like dice—is, if confirmed, of the greatest interest and importance.

There would still be alternative ways of interpreting the responses of other organisms to human volitions. We *might* suppose, for example, that the organisms are exercising powers of ESP! Although this sort of speculation seems incapable of verification by experiment, we ought perhaps to keep it in mind. Far-fetched though it may appear to attribute 'psychic' powers to unicellular organisms like paramecia, we should not take it for granted that organisms are as passive as we suppose dice to be in responding to human volitions.¹

C. W. K. MUNDLE

Department of Philosophy,
University College, Dundee.

'TELEPATHY AND SPIRITUALISM'

SIR,—In view of Dr West's past denigration of my contributions to psychical research, his review of my latest volume *Telepathy and Spiritualism* did not surprise me; I must confess, though, that I was rather taken aback by the undignified way he attacked me and my work.

'He has a bee in his bonnet about statistics, which he obviously does not grasp', says Dr West. As regards the first part of this stab, I console myself with the fact that human society is full of people who have more than one bee in their bonnet, whose produce is in inverse ratio with the great noise of their buzz. As regards its second part, the stab is thrust because of my suggestion that the cardguessing results would be rendered more acceptable if the tests were carried out with a 'control', and because of my disbelief in the 'precognitive telepathy' interpretation in cardguessing and that of the results in dice-throwing experiments. Dr West shuts his eyes to the fact that this disbelief is extensively entertained in scientific and other quarters, including Dr Gilbert Murray, the President of the S.P.R. (see his Presidential Address in the concurrent number of the *Proceedings*). He should also ponder on the following opinion expressed by John Hopkinson: 'What is wanted is not to depend upon mathematics alone, but to have a general appreciation whether the thing looks about right, otherwise mistakes will be made, however perfect the mathematics may be.'

¹This letter was written before the publication of the S.P.R. *Journal* for November-December 1952, i.e. before Mr Mundle had read the paper by Professor Lossy, on pp. 707-8 of which a very similar point is made.—ED.

Dr West obviously believes in attack being the best method of defence. Hence, one of his further charges is my 'attitude of juvenile animosity towards other investigators', whereas, as his review clearly shows, the boot is on the other foot. I hope that readers of the *Journal* will also read Chapter II in my book, and judge for themselves whether the views I expressed in support of my disbelief show any animosity, and whether they make 'droll reading', as Dr West further asserts. 'It is a tragedy that years of effort were wasted on these faulty experiments' is another assertion. The reference to 'tragedy' has not a true ring, and as to the experiments being 'faulty', this is a sweeping assertion convenient to a defensive attack but not a victorious weapon. 'Much of his research has been demonstrated to be ill-founded', says Dr West. Demonstrated by whom? I suppose he refers to Christopher Scott's thirty-five pages long review in the *Proceedings* of November 1949, in which thanks are acknowledged to Dr West for his 'continued help and encouragement' (see page 18 of that issue). Impartial readers of that review could not have failed to notice the spirit of animosity and tactless (to say the least of it) insinuations pervading it. I submit that, in such circumstances, no arguments are reliable in the demonstration of any alleged truth, coming as they do from an unfriendly quarter. Furthermore, speaking of 'faulty' and 'ill-founded' experiments, alleged by Dr West, and his suggestion that they would have been prevented by taking 'friendly advice' from other investigators, let me point out a fact not mentioned by Scott in his review. Scott changed to the so-called 'control' pictures *after* he had received my report of the sensitive's impressions. Does Dr West suggest that Scott could not have changed the pictures, by comparing them with the impressions he has been made acquainted with, in such a way as to prove what he wanted to prove? Or does he consider that allegations of such character can be brought against me, as Scott did, but not against him, because he is to be trusted but I not?

And now, a few remarks about two misleading assertions. Dr West lets one infer that the pre-arranged pictures in my attempts at telepathic signalling is at par with the use of the five designs in cardguessing. It is nothing of the kind, for in cardguessing the percipient knows the pictures he has to guess, while in my experiments the sensitive has not the slightest idea what they are. He also lets one conclude that I believe in precognition being due to spirit intervention. This, too, is not an accurate rendering of what I wrote under the title 'The Problem of Precognition'. First of all, I did not say that I definitely accept precognition to be a reality (see page 80), and secondly, I made a reservation about my

belief in spiritualism (see page 113). My statement that I prefer a spiritualistic hypothesis of precognition to one adverse to common sense in which an effect is supposed to precede its cause, was thus made dependent upon both spiritualism and precognition being proved to be beyond any possible doubt.

In the last paragraph of his review, Dr West considers my experiments to be 'excellent in conception yet faulty in execution'. I find consolation in the first part of this opinion, for, in the nearly fifty years in the practice of my profession, I have met a very great number of scientists and inventors, whose conceptions were excellent, yet many years had to pass before they were carried out perfectly in practice by their originators or by others who believed in those conceptions.

J. HETTINGER

London, S.W. 16.

Dr West writes : Dr Hettinger seems to have taken my review of his book as a personal attack. I am sorry, for my concern is only to criticise his methods and opinions.

Dr Hettinger's letter contains no answer to the fundamental criticisms of his experiments that have been expounded at length by Christopher Scott. It is not answering the criticisms to assert that they were made by Scott in a spirit of animosity, and it is a poor answer to the negative result of Scott's control test to suggest that he might have produced it by cheating. We have got beyond the stage of suspecting every experimenter of cheating, and neither Mr Scott's honesty, nor Dr Hettinger's, need be questioned.

My remarks about Dr Hettinger's peculiar outlook on statistical evaluation seem to me justified. John Hopkinson's words are very sensible, but they do not lead me to Dr Hettinger's position. Anyone inspecting the record sheets from subjects like Basil Shackleton can see, without working out the exact odds statistically, that something extraordinary is happening. Although chance expectation in card-calling tests is given reliably by formula, there have been many 'control' tests, a fact of which Dr Hettinger seems unaware.

I did not intend to use the word 'tragedy' sarcastically. There are so few experimenters in our subject, and their time is so limited, that to have one of them spend years on badly executed research is nothing less than a tragedy. I can only reiterate the hope that in his future research Dr Hettinger will have any technical difficulties straightened out by consultation at an early stage.